

# ***Interactive comment on “Assessment of cloud supersaturation by aerosol particle and cloud condensation nuclei (CCN) measurements” by M. L. Krüger et al.***

## **Anonymous Referee #2**

Received and published: 16 January 2014

### General comments

The paper present novel techniques to measure in-cloud supersaturation. This parameter is of great interest in order to understand and model cloud formation. A well-functioning method to measure this would be a large instrumental improvement in cloud science. No direct method to measure in-cloud supersaturation (or relative humidity, RH) exist. Methods using CCN counters seem promising. This paper present some first attempts, and even if the methods not yet are optimised, a promising outline for improvement is presented. Especially using faster scanning of supersaturation in the CCN counter (Moore and Nenes, 2009).

C4003

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



As far as I understand, the goal is to develop methods to measure in-cloud, in-situ peak supersaturations (or RH). The methods described are attempts to that, except the “Hoppel minimum” method (sect 3.2.2). That method is more a measurement of the history of the aerosol, i.e. the supersaturation that the aerosol was exposed to in previous cloud passages. Results from the “Hoppel minimum” method can anyway be interesting for comparison, but I cannot see that as a relevant method for in-situ measurements. Or maybe I misunderstood something here.

The spread in results, see Table 2, seem rather large. The authors state in section 4 “Conclusions and outlook” that the uncertainties mostly depend on limitations in time resolution and counting statistics, as well as uncertainties in the aerosol hygroscopic properties for the SMPS methods. It might be interesting to also have a discussion and draw conclusions about the reliability of the different methods. Are all methods equally accurate?

The authors discuss already in the abstract the variability of supersaturations that the aerosol particles and cloud droplets are exposed to during the evolution of the cloud. How does this affect the results? Is it only a time variation?

#### Specific comments

The section starting on page 10024, line 27, reaching until page 10025, line 3 (Most likely ...) is most likely correct, but seem not very relevant to this paper. The techniques presented aim to measure supersaturation in a specific cloud at a specific time and location, i.e. the location where the instrumentation (inlet) is placed. Earlier cloud passages by the aerosol particles seem not very relevant to that parameter.

I believe section 4 “Conclusions and outlook” could be improved, especially the conclusion part. It would be interesting if you could present a few more conclusions from your work, see also above.

#### Technical corrections

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Page 10023, line 11 and page 10046, line 23: Is “Pruppacher and Klett, 2010” really correct? My version is from 1997.

---

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 10021, 2013.

**AMTD**

6, C4003–C4005, 2014

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C4005

